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## INDIVIDUAL DISCRIMINATION BY OLFACTORY CUES IN MICE (*MUS MUSCULUS*): A MULTIPLE CHOICE CONFIRMATION

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**ABSTRACT:** The ability to discriminate four individual conspecifics by olfactory cues in laboratory mice was investigated. Adult male mice of the CD-1 outbred strain were water deprived and placed in a Plexiglas four-arm maze, twice a day, five days a week. Subjects were assigned to two groups: Training and Control. Training mice were trained by water reinforcement (a drop of water) to choose the maze's arm containing the sawdust of one of four donors (adult males, same strain), while for Control mice the association between sawdust and reinforcement was continuously varied. Data collected during three weeks showed that Training mice made significantly fewer errors in finding the water than Control mice, thus confirming the results of previous experiments based on a two-way choice that proved mice to be able to discriminate conspecifics individually by olfactory cues.

**RIASSUNTO:** Si è esaminata la capacità di topi di laboratorio di discriminare individualmente quattro conspecifici mediante la percezione olfattiva. Topi maschi adulti del ceppo non consanguineo CD-1 sono stati privati di acqua e posti in un labirinto a 4 bracci di Plexiglas, due volte al giorno, per cinque giorni alla settimana. I soggetti sono stati divisi in due gruppi: Training (Addestrati) e Control (Controlli). I topi Training sono stati addestrati a trovare una ricompensa costituita da una goccia di acqua, tale ricompensa era sempre associata a una solo tipo di segatura tra le quattro presenti (i quattro diversi donatori erano dello stesso ceppo), mentre per i topi Control l'associazione tra segature e ricompensa era continuamente variata. I dati raccolti nel corso di tre settimane hanno mostrato che i topi Training hanno effettuato un numero significativamente minore di errori nel raggiungere l'acqua rispetto ai topi Control (Controllo), confermando così i risultati di precedenti esperimenti basati su una scelta binaria che evidenziavano come i topi fossero in grado di discriminare individualmente dei conspecifici sulla base di segnali olfattivi.

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## INTRODUCTION

Olfactory cues play an important role in the social behaviour of mice (Bronson, 1979; Brown, 1979; Hurst, 1990a, b, c). Mice can use odours to discriminate among closely-related strains (Hahn & Simmel, 1968) and to recognize gender (Bowers & Alexander, 1967), reproductive condition (Hayashi & Kimura, 1974) and social status of individual conspecifics (Jones & Nowell, 1973; Bishop & Chevins, 1987). Moreover, among mice, both mating and aggressive behaviour seem to be greatly influenced by conspecific odour cues depending on the social group (own/alien) and/or degree of kinship (Bronson, 1979; Barnard, Hurst & Aldhous, 1991).

Urine is considered the most important source of socially meaningful odours (Archer, 1968; Jones & Nowell, 1974; Barnard & Fitzsimons, 1988; Gosling & McKay, 1990; Novotnoy, Harvey & Jemiolo, 1990; Krackow & Matuschak, 1991). Urine marks seem to possess individual characteristics that appear to be determined partially on a genetic basis (Beauchamp, Yamazaki & Boyse, 1985; Jemiolo et al., 1991). According to several authors, identity signals can be correlated with differences in the polymorphic loci of the major histocompatibility complex (MHC) genes, since mice can be trained to discriminate between urine of individual congenic conspecifics that differ only in the MHC (Yamazaki et al., 1979; Yamaguchi et al., 1981; Beauchamp, Yamazaki & Boyse, 1985). Environmental factors (i. e. diet, microbial fauna) have also been proved to participate in the formation of a distinctive urinary odour (Schellinck, West & Brown, 1992).

The methods currently employed to demonstrate individual discrimination involve a series of two-way choices between two individual conspecifics which do not differ in either age or sex (Halpin, 1986). Mice to be discriminated are both unfamiliar in the positive reinforcement paradigm, firstly used by Bowers & Alexander (1967), while differences in the familiarity with the stimulus subjects represent the critical factor in the habituation-discrimination paradigm (e. g., Kimelman & Lubow, 1974). The ability to discriminate individual conspecifics both in rodents and in birds has been proved to be better evaluated when animals are presented with a multiple choice in place of a two-way choice (Haynes & Purvis, 1972; Gilder & Slater, 1978; Bateson, 1982). The aim of the present experiment was to investigate the extent of individual discrimination when mice are given choices between four unfamiliar stimulus animals.

## METHODS

### *Animals*

Twenty-eight outbred albino Swiss (CD-1) male mice (mean age 70 days) purchased from Charles River (22050-Calco, Italy) were used. On arrival, twenty-four of them (Subjects) were marked with picric acid and housed in groups of five individuals in 42x27x15 cm Plexiglas cages with stainless metal tops (Tecniplast, 21020-Varese, Italy). The remaining four (Donors) were individually housed in similar cages differing only in dimensions (33x13x14 cm). Bedding consisted of sawdust obtained from white and red fir-wood, free from chemical solvents, and containing neither resin nor dust (FTS, 20136-Milano, Italy). The housing room was air-conditioned at  $21 \pm 1^\circ \text{C}$  and 70% relative humidity, with a 12-12 red light/white light cycle (red light on at 9:30 a.m.). Pellet food (Enriched standard diet, D. Piccioni, 25100-Brescia, Italy) and tap water were continuously available. The sawdust was removed twice a day, 30 min before use, and replaced with an equal amount of clean sawdust. The collected sawdust was maintained at room temperature in a light-proof plastic box to prevent olfactory changes from occurring in the course of the experiment.

### *Apparatus*

The apparatus was a grey Plexiglas maze (radial, four-arm) raised 50 cm above the floor. Each of the arms was 32 cm long and 4 cm wide. The maze walls were 4 cm high. A box (4 x 4 x 0.5 cm) was placed in each arm, 4 cm away from its opening onto the central platform (circular, diameter 13 cm). The maze was covered with a removable, transparent Plexiglas top with a central round opening (6.2 cm wide), supplied with a cover. The maze was placed in a quiet experimental room under temperature, relative humidity, and lighting conditions very similar to those of the housing room.

### *Procedure*

After one week each subject was given a 10-min familiarization trial in the maze and then placed on a 23-h water-deprivation schedule. Twenty-four h later, each subject was given two 10-min familiarization trials with a between-trial time lapse of 3 h. One drop of water (50  $\mu\text{l}$ ) was then placed in a small hole (1 cm deep) at the end of a randomly-chosen arm. Following the testing of each animal, the maze was

thoroughly cleaned with a solution of alcohol and water (50%) and dried. During familiarization trials the boxes were empty.

The experiment started 24 h after familiarization, and took place during the dark period (between 12:00 and 20:00 hours). All mice were given two trials a day (between-trial interval: 3-4 h), 5 consecutive days a week. The boxes were filled with soiled sawdust from the donor animals (1-2 g). Each box contained sawdust from a different donor. The mice were randomly assigned to one of two groups: Training (16 subjects) or Control (8 subjects). Training mice always found the reinforcement (water) associated with the same sawdust, while for the Control mice water was randomly associated with one of the two types of sawdust. A greater number of subjects was used for the Training group as a more heterogeneous response was expected in this group than in the Control group. Indeed, we did not know if one of the sawdust types was easier or more difficult to discriminate than the others.

The order of testing was random. The relative positions of the boxes were varied between trials, and the boxes also moved with respect to extramaze cues. Before each trial, the maze and the boxes were wiped with an alcohol/water solution. Individual mice were placed on the central platform and the trial began when the animal touched the platform with its four paws. An error was counted when the subject passed beyond a box in an arm without reinforcement. During each trial, the number of errors was scored and the time to reach the reinforcement was recorded by a silent hand-held stopwatch. Each test was ended either when water was reached or after 3 min (whichever was first). This cut-off time was seldom exceeded.

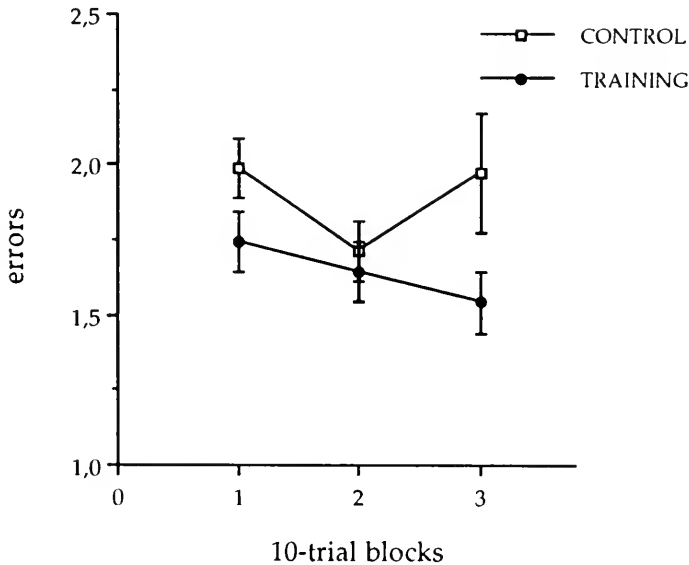
During the water-deprivation phase, we continuously checked the mice for signs of distress. None were found.

## RESULTS

On the first experimental day, Control and Training mice made the same number of errors before reaching the water (i. e. 1.7 errors). However, in the following days, data show that Training subjects made fewer errors than Control mice before reaching the water reinforcement. An ANOVA (2 x 3) with a between-groups factor (Control and Training) and a within-groups factor (three repeated 10-trial blocks, each block grouping one week) proved this difference to be significant [ $F(1,22) = 5.24, p = 0.03$ , see Fig. 1]. Analysis did not reveal an effect due to the interaction between group and block number.

The data of the Training group could be analyzed as a whole since

repeated ANOVA ( $4 \times 3$ ) measures found no significant differences between the performances of the four subgroups of the Training group [ $F(1,22) = 1.79, p = 0.17$ ].



**Figure 1.** Mean ( $\pm$  SE) level of errors before reaching reinforcement scored for C (Control) and T (Training) subjects during three 10-trial blocks.

## DISCUSSION

A recent study in a semi-natural setting (Hurst, 1993) has stressed the role of individual discrimination by olfactory cues in social communication among male mice living in the same area. In laboratory studies, this ability has been demonstrated by experiments involving two-way choice (Bowers & Alexander, 1967; Hahn & Simmel, 1968; Kimelman & Lubow, 1974; Yamaguchi et al., 1981; Beauchamp, Yamazaki & Boyse, 1985; Gosling & McKay, 1991).

Giving animals multiple choices is more likely to reveal the existence of a discrimination process (Gilder & Slater, 1978; Bateson, 1982). Our experiment verified that the ability to discriminate individuals by olfactory cues is also expressed in mice presented with multiple choices. The discrimination curve was not clear however, possibly due to the limited number of trials (see Bowers & Alexander, 1967; Yamazaki et al., 1979) which was agreed upon with our intramural

Ethical Committee before the beginning of the study.

We considered urine odour to be the basis of the discrimination reported. But since the odour source was represented by sawdust we cannot exclude an effect due to other organic components present (secretion of specialized exocrine glands, feces, hair), that have been shown to affect behaviour (Apps, Rasa & Viljoenet, 1988; Goodrich, Gambale, Pennycuik & Redheadet, 1990a, b; Marchlewska-Koj, Pochron & Sliwowska, 1990).

## ACKNOWLEDGEMENTS

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## THE MATCHING LAW IN HAMSTERS

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and Shigeru Watanabe

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**ABSTRACT:** Most studies of the matching law have used pigeons or rats. Hamsters usually do not consume food immediately but store it in their cheek pouches. In the present experiment, three hamsters were trained on Concurrent Variable Interval-Variable Interval (Conc VI-VI) schedules with food reinforcement in an operant chamber with two levers. The value of the VI schedule was changed from 10 s to 90 s. A linear regression of log reinforcement ratio to log response ratio described the choice behavior of the hamsters well. These results suggest the applicability of the generalized matching law to operant behavior that is not immediately followed by consummatory behavior.

### INTRODUCTION

Herrnstein (1970) described a matching relationship between relative frequency of operant responding and relative frequency of reinforcement when two independent schedules of reinforcement were concurrently effective. Since then, there have been many studies demonstrating robustness of the matching law (Baum, 1979; Wearden & Burgess, 1982). The matching relationship has been obtained not only with food reinforcement but also with aversive reinforcers (Hutton, Gardner & Lewis, 1978) and brain stimulation (Gallistel, 1969). Although the matching law has been applied to cows (Matthews & Temple, 1979), monkeys (Liewellyn, Iglauer & Woods, 1976), dogs (Rashotte & Smith, 1984), free ranging wild pigeons (Baum, 1974a) and wagtails in the wild (Houston, 1986), most studies of matching have used rats or pigeons. Hamsters have a peculiar feeding pattern in that they do not consume food immediately but keep it in their cheek pouches, and consume it usually when they return to their nest. Hamsters also show food

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hoarding behavior (Phillips, 1989; Wong, 1984). Therefore, a food pellet given to a hamster as a reinforcer may not be food for immediate consumption but food to pouch for hoarding.

The purpose of the present experiment was an examination of applicability of the matching law to this particular species. The relationship between the response rate and the reinforcement rate were expressed as a generalized matching law (Baum, 1974b) described as:

$$\text{Log}(B1/B2) = A \times \text{Log}(r1/r2) + B$$

where B1 and B2 are the response rates for two levers and r1 and r2 are the reinforcement rates obtained. Strict matching means that A equals 1.0 and that B equals 0.0. There are two types of deviation from the strict matching. When A is less than 1.0, the deviation is called undermatching. Undermatching means insensitivity to changes of relative reinforcement frequency, that is, the animals respond on a less reinforced lever more often than expected from relative frequency of reinforcement. When A is more than 1.0, the deviation is called overmatching. This means oversensitivity to changes of relative reinforcement rate whereby animals prefer a rich lever more than expected from the relative frequency of reinforcement. B gives a bias of responding. If animals have some preference for one lever regardless of frequency of reinforcement, B deviates from zero. The response pattern of hamsters was analyzed using the generalized matching law described above.

## METHOD

### *Animals*

Two male and one female experimentally naive golden hamsters (*Mesocricetus auratus*) were used. They were 6 months old at the start of the experiment and were maintained at 80 percent of their free-feeding weights. The room temperature was approximately 22° C throughout the experimental period. Illumination of the cage room was via a 12 h light-dark cycle.

### *Apparatus*

The apparatus was an operant chamber (20 x 30 x 20 cm) designed for hamsters. There were two response levers 10 cm apart on the front

panel. The distance from the floor to the lever was 1.5 cm. A 25 gm weight would activate the levers. A pellet dispenser, which delivered a 40 mg pellet (Muromachi Kikai), was located at the center between the levers. There was a miniature lamp (24V, DC) on the ceiling of the chamber. A random noise generator (Rion, SF-05) continuously produced white noise (70 dB), and a microcomputer system (Sanyo, MSX+) determined the experimental contingencies.

### *Procedure*

The hamsters were first shaped to press either lever. Then, a Variable Interval (VI) schedule with a single lever was introduced. During the single lever training, the unused lever was covered with a plastic box. The value of the VI schedule gradually increased to VI 30 s. After the hamsters showed stable responding on either lever, training on a Concurrent Variable Interval - Variable Interval (Conc VI-VI) schedule began. A changeover delay (COD) of 2 s was introduced into this concurrent schedule whereby a reinforcer on one lever was not available until 2 s had elapsed from the last response on the other lever. The daily training session lasted 30 min.

Table 1 shows the value of the concurrent schedule. The animals received Conc VI-VI training with a COD for 5 to 7 sessions. Stability of responding was evaluated using the relative number of responses to lever 1 (the left lever), calculated by dividing the number of responses to lever 1 by the total number of responses to both levers. When the animals did not show stable responding during the initial 5 or 7 sessions, the training was prolonged until fluctuations of the relative response measure remained less than 10 % for 3 successive sessions. The schedule value was then changed to the next one. However, one animal (M3) changed to the next schedule without satisfying this criterion due to the high variability of its responding. Pouching was determined and recorded after each daily training session. The pellets were not removed from the pouch.

## RESULTS

Table 1 shows the number of sessions for each schedule value and the range of variability in the final three sessions of each condition. Mean response rates for one lever in the final three sessions on conc VI30"- VI30" was 4.81 / min, 3.87 / min and 9.34 / min for M1, M3 and F3 respectively. Each animal pouches the pellets obtained during

TABLE 1

**Schedule values and number of sessions. L and R indicate the left and the right levers respectively. The number in brackets indicates range of percentage fluctuations of relative responding to the left lever over the total responses in the final three sessions.**

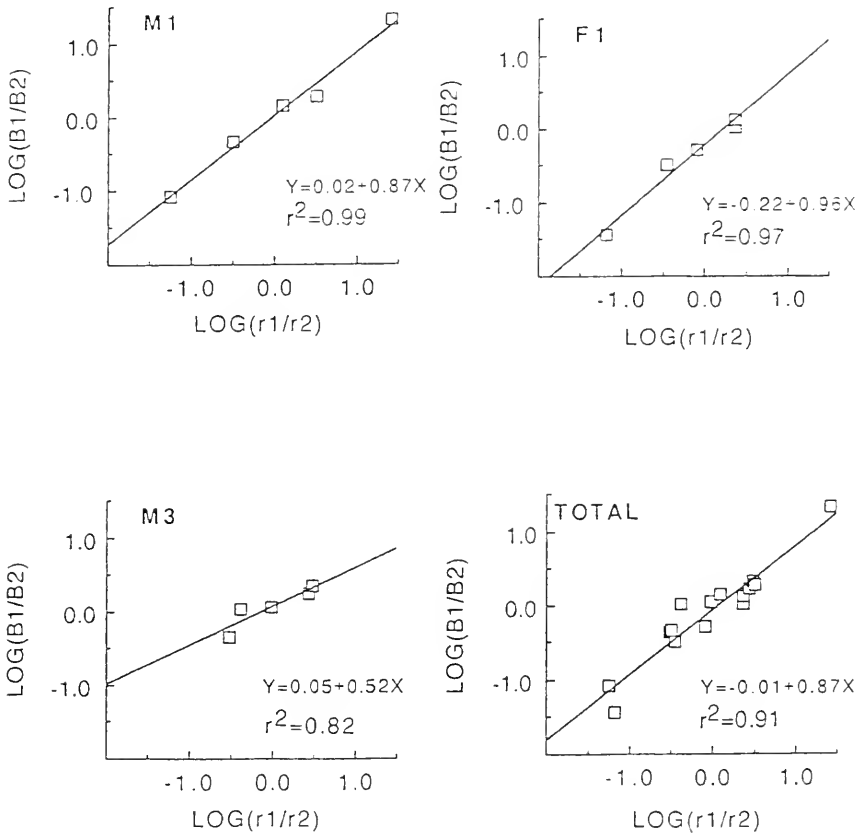
| M1 |    |          | M3 |    |          | F1 |    |          |
|----|----|----------|----|----|----------|----|----|----------|
| L  | R  | Sessions | L  | R  | Sessions | L  | R  | Sessions |
| 30 | 90 | 5 (3)    | 10 | 30 | 5 (6)    | 30 | 10 | 7 (5)    |
| 30 | 10 | 6 (18)   | 90 | 30 | 7 (3)    | 30 | 90 | 13 (2)   |
| 30 | 30 | 24 (26)  | 30 | 30 | 9 (9)    | 30 | 30 | 6 (9)    |
| 10 | 30 | 6 (7)    | 30 | 90 | 7 (3)    | 90 | 30 | 6 (5)    |
| 90 | 30 | 7 (3)    | 30 | 10 | 7 (2)    | 10 | 30 | 6 (2)    |

the daily training sessions. They were not seen to eat the pellets in the operant chamber but instead consumed them after they came back to their living cages.

Figure 1 shows mean relative responding in the final three sessions of each schedule value. The results support the generalized matching law. The  $r^2$  value for the regression was between 0.82 and 0.99. The value of A was less than 1.0 for each hamster, so that they showed undermatching. F1 preferred lever 2 (the right lever) but the other animals did not show a strong bias.

## DISCUSSION

The present results clearly demonstrated successful expansion of the generalized matching law to choice behavior of hamsters. The slope of the regression (0.87 for the three animals) was similar to data obtained from pigeons (0.84 for 137 birds, Baum, 1974b). Undermatching has been observed in most of the experiments on the matching law (Baum, 1979; Wearden & Burgess, 1982). The present results support this general tendency.



**Figure 1.** A generalized matching law analysis of choice behavior in three hamsters on Conc VI-VI schedules. B1 and B2 indicate response rates on the left and the right levers respectively. r1 and r2 indicate reinforcement rates obtained from each lever. The equations are those of the best-fitting straight lines obtained by the method of least squares.

In conventional operant conditioning with food reinforcement, there is a behavioral link between the presentation of food and its consumption. This link operates without interruption in conventional operant experiments with rats or pigeons. On the other hand, the particular feeding behavior of hamsters produces a delay between obtaining food and its consumption. Shearon and Allen (1989) suggested the existence of schedule induced pouching in hamsters. Thus, the pouching observed in the present experiment may have been facilitated by the schedule itself. The observation that three hamsters showed pouching in all training conditions suggests the applicability of the matching law to operant behavior in animals with this particular

consummatory behavior pattern. There are two possible explanations for the matching relation maintained by pouching. First, pouching may function as a conditioned reinforcement because hamsters obtain the primary reinforcer after pouching. This explanation, however, does not provide a reason for pouching. An alternative explanation is that the emission of pouching behavior itself is reinforcing for hamsters. Emission of innate behavior, such as aggression, has reinforcing effects in fighting fish (Thompson, 1963) and fighting cocks (Thompson, 1964). For hamsters, it could be conclusive to remove pouched pellets after completion of their daily training sessions.

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## INDIVIDUAL DIFFERENCES IN THE BEHAVIOR OF ALBINO AND WILD HOUSE MICE (*MUS MUSCULUS*)

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**ABSTRACT:** Adult male and female albino (AA) and wild (WW) housemice were individually evaluated in the laboratory for their performance in exploration (EX), insect predation (PD), burrowing (BW) and food hoarding (HD) activities. The results showed that (a) in both AA and WW adult mice there are individual differences (IDs), tending to be stable at least for periods of 60 days; (b) WW tended to show higher frequencies of the activities (WW males in EX and WW females in EX, PD, and HD); (c) significant sex differences occurred both in AA (with females scoring higher in EX and BW, and males in HD) and in WW (with higher female frequencies of burrowing); (d) individual animals had distinct combinations of performance in the four behavioral activities, suggesting independence among the motivational systems responsible for the regulation of these activities.

**RESUMO:** Camundongos albinos (AA) e selvagens (SS) adultos - machos e fêmeas - foram testados individualmente em laboratório quanto a seus desempenhos na atividade exploratória (EX), predação de insetos (PR), escavação de subterrâneo (ES) e armazenamento de alimento (AR). Os resultados mostraram que (a) em camundongos adultos, albinos e selvagens, existem diferenças individuais (DIs), estáveis pelo menos para períodos de 60 dias; (b) os SS apresentaram tendência a um maior desempenho (machos SS em EX e fêmeas SS em EX, PR e AR); existem diferenças intersexuais, estatisticamente significantes, tanto entre os AA (tendo a fêmeas apresentado maior desempenho em EX e ES, e os machos em AR) quanto nos SS (as fêmeas com maior desempenho em ES); (d) os animais têm combinações distintas de desempenhos nas quatro atividades avaliadas, o que sugere uma independência entre sistemas motivacionais responsáveis pela regulação destas atividades.

The importance of individual differences in behavioral

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performance is being increasingly acknowledged not only from a methodological (Martin & Kraemer, 1987), adaptive and evolutionary standpoint (Hirsh & McGuire, 1982) but also from a physiopathological standpoint (Koolhaas & Oortmerssen, 1988).

Individual differences (IDs) have been described in several mammalian and nonmammalian species (see Negrão & Schmidek, 1987 for references). In most of these studies only a single behavior is analysed. In a previous paper (Negrão & Schmidek, 1987) our laboratory has shown marked and stable IDs in four biologically relevant behavioral activities in rats namely burrowing, food hoarding, exploration and insect predation. The simultaneous analysis of several behavioral activities proved to be important. Individual performance in each of these activities was independent, suggesting that the process of individualization involves a specific modulation of independent motivational systems, as has already been suggested by Tryon (1934) and others more recently.

In the present experiment, individual performance in the same four behaviors was measured in adult male and female, albino and wild housemice. The following assumptions were made: (a) individual animals would show markedly different performances in each of these behaviors; (b) individual performances in different behaviors would be independent and thus not a mere consequence of more general phenomena such as level of activity; (c) males and females as separate groups would show different performances; (d) albino and wild mice as separate groups would show different performances.

## METHODS

### *Subjects*

Albino and wild mice of both sexes were used in the experiment. Albinos (20 males and 20 females aged 90 days at the beginning of the experiment) were from different litters of a heterogeneous breeding stock kept in the animal facilities of our campus. Wild housemice (6 adult males and 6 adult females) had been trapped in a rice storage area 3 or more weeks prior to the first test series. The animals were individually housed in wooden cages (20 x 30 x 15 cm), with wood wool (replaced weekly), and *ad libitum* water and food (loose Purina rat chow and supplements) in the cage. The light cycle was 12:12 h (lights on at 6:30 a.m.); the temperature varied from 20°

to 26°C.

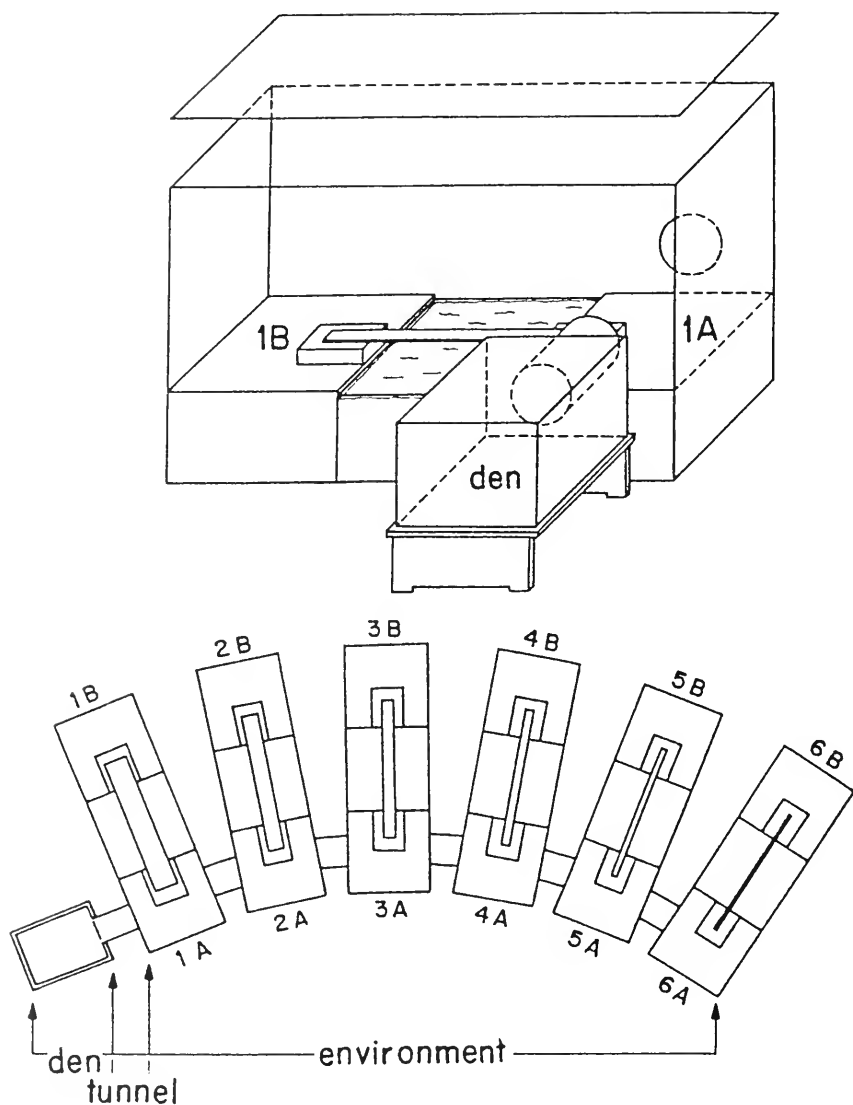
### *Apparatus*

Four different types of equipment were used to evaluate the performance of individual animals in the four behavioral activities relevant for the species under study: exploration, insect predation, burrowing and food hoarding. This methodology is similar to that previously used with rats (Negrão & Schmidek, 1987), but now adapted to mice.

Exploration was evaluated in a multichamber system (Fig 1) in which one cage (a 15 x 25 x 10-cm wooden box with a translucent glass lid) acted as a den compartment. This cage was connected to a complex environment by means of a tunnel comprising a rigid plastic tube, 10 cm long and 7.6 cm in diameter. The den and environment were discriminable by both odors and brightness. The den contained only self-produced odors (Hughes, 1991) and was darkened by a translucent lid. The complex environment consisted of 12 intercommunicating compartments (20 x 20 x 25 cm each). The first of these (1A) was also connected to the den. Six of these compartments (1A-6A) were connected in series through 7.6 cm diameter tubes to corresponding compartments (1B-6B) of the same size. Each pair of corresponding A and B compartments was separated by a water tank (20 x 20 cm in surface area and 10 cm deep). This tank could be crossed by swimming, jumping or, as almost always happened, by means of runways of different widths, ranging from 1.8 to 0.2 cm.

Insect predation was tested with the animal in a 50 x 25 x 8-cm wooden cage with a transparent glass front wall and cover. Five intact immature (approximately 2 cm long) cockroaches (*Periplaneta americana*) raised in our laboratory were used as prey. Before the test, the five insects were placed in a 20-cm long, 5.1-cm wide tube obstructed at the proximal end with a sliding lid and at the other end with a piston. At the beginning of the test this tube was attached to an opening on the side wall of the arena, the sliding lid was removed, and the cockroaches were pushed into the arena with the piston.

Burrowing tests were performed in a 120 x 25 x 25-cm terrarium containing 10 cm of loosely packed dampened soil that was replaced before each session by the following procedure: after each test the top layer was scraped away and discarded; the remaining soil was transferred to a large container and thoroughly mixed with soil of controlled humidity (between 200 and 260g water per 1000g of dry



**Figure 1.** Schematic illustration of the multichamber exploration set. The upper figure shows a lateral view of the den and the first pair of A and B environmental compartments separated by a water tank. The lower figure shows a top view of the whole system.

soil); the terrarium was then filled again with soil from this container. An opaque platform (85 x 25 cm in surface area and 1 cm in thickness) was placed on the surface. Excavations were usually performed under the platform starting at its edge. This platform prevented cave-ins, and permitted an adequate evaluation of the

burrowing activity.

Hoarding tests were carried out in a set of two interconnected cages. One, designed to work as a den compartment, was a 25 x 25 x 10-cm plywood box, closed, except for a rigid plastic tube, 25 cm long and 3.6 cm in diameter, that enabled access to a 50 x 35 x 35 cm glass-walled and wire mesh-covered cage. A reticulum of lines, forming 5 x 5-cm squares was drawn on the ground of both cages. Hoarding materials were offered in the environment cage at the beginning of the test. These materials consisted of 33 food pellets placed in a checkerboard pattern (one pellet in each alternate square of the reticulum) interspersed with nesting materials (20 small twigs, 15 round gravel-stones) and two handfuls of wood shavings.

### *Procedure*

Each mouse was given the test battery three times each being separated by 30 days. Each battery began at 10 a.m. on the first day (Test I), when the animal was identified, weighed, and housed in the den compartment of the exploration set. We started this test 1 h later by opening the tunnel and by recording the whereabouts and behavioral patterns of the animal during a 15-min period with the use of an event recorder specially designed for this purpose (Schmidek et al, 1983). At 6 p.m. on the same day, the animal was transferred to the predation cage in which the insect predation test was to be performed 24 h later. During this period of adaptation to the cage, the animal had no other food except for a live immature cockroach and *ad libitum* water. The predation test on 6 p.m. of the second test day started immediately after the delivery of the five cockroaches into the cage and lasted 15 min, during which time the mouse's behavioral activities were recorded with the event recorder. At 9 a.m. on the third day, the mouse was transferred to the terrarium for the burrowing test. This test ended on the fourth day at 9 a.m. when a topographical map of the surface and underground of the terrarium was drawn. At 10 a.m. on this fourth day, the mouse was transferred to the hoarding cages and at 4 p.m. on the same day, food and nesting materials were offered as described. At 10 a.m. on the fifth day we ended the hoarding test by recording the new arrangement of materials in both compartments. At the end of each test, the cages were washed and dried and the soil in the terrarium was replaced as described above. The second and third series of tests (Test II and III) were begun 30 and 60 days later respectively.

## RESULTS

Performance by males and females, and between both strains were compared via the Mann-Whitney  $U$  test and  $\chi^2$  test. Agreement among individual performances in the first, second and third series of tests was calculated via the Kendall  $W$  concordance analysis (Siegel, 1956).

Range and medians in both experimental groups are shown in Table 1. It was apparent that different animals exhibited widely different performances. Although some showed low performance in the exploration (EX) test (e.g., albino male AM33 with 1.1, 2.4 and 2.0 minutes of active exploration in tests I to III, or albino female AF44 with 3.7, 3.9 and 4.4 minutes in these tests), most of them showed intense exploratory activity (e.g., albino male AM09 with 5.8, 6.5 and 6.3 min. and albino female AF22 with 6.5, 7.3 and 7.4 min, respectively). In the insect predation test (PD) there were some animals that did not kill any cockroach (AM11, AM15, AM35, AM37, AM45, AF14, and AF46) whereas others killed three or more in each test (albino mice AM23, AM29 and AF38 and wild mice WM11, WF01 and WF06). In the burrowing test (BW) there were some mice, especially albino males that did not burrow in any of the three series of tests (AM13, AM17, AM29, AM39, AM45, and AF38). However, most animals, even in the first test series, excavated burrows - some of them quite large (e.g., AM09 with 775, 1400 and 1400 cm<sup>2</sup>, AF30 with 900, 1475 and 1500 cm<sup>2</sup>, WM13 with 500, 775 and 1300 cm<sup>2</sup>, WF02 with 1100, 1000 and 1625 cm<sup>2</sup>). Also in the hoarding test (HD), individual performances were widely contrasting. Some mice carried most of the food pellets to the den compartment (e.g., AM19 that carried 54.8, 97.0 and 81.3% of the pellets to the den compartment, AM47 with respectively 78.8, 69.7 and 69.7%, WF08 with 69.7, 100.00, 51.5% or WF10 with 66.7, 81.8 and 63.6%) whereas others showed no hoarding behavior at all (AM39, AM43, AF12, and AF34).

Individual performance in the three test series tended to be stable in albino animals with a statistically significant concordance in each of the four behaviors. Values of Kendall concordance coefficient  $W$  in albino male mice were .726 for EX, .814 for PD, .790 for BW and .699 for HD. In Albino females these values were .776, .567, .769 and .758 respectively for EX, PD, BW and HD. All these values are statistically significant for  $p < .05$ . Despite the reduced numbers of wild mice, significant concordance was detected in BW males ( $W = .699$ ) and in EX and HD females ( $W = .840$  and .936, respectively).

TABLE I  
Range and median values of behavioral performance in both experimental groups

| SERIES | DURATION ACTIVE EXPLORATION (Min) |         |         | NUMBER OF COCKROACHES KILLED |         |         | UNDERGROUND BURROWING AREA (Area=cm <sup>2</sup> ) |          |          | PELLETS IN DEN (%) |            |          |
|--------|-----------------------------------|---------|---------|------------------------------|---------|---------|--|----------|----------|--------------------|------------|----------|
|        | I                                 | II      | III     | I                            | II      | III     | I  | II       | III      | I                  | II         | III      |
|        | ALBINO MICE - Males (N=20)        |         |         |                              |         |         |  |          |          |                    |            |          |
| Range  | 1.1-7.2                           | 1.4-7.3 | 2.0-6.3 | 0.0-5.0                      | 0.0-5.0 | 0.0-5.0 | 0-1050   | 0-1400   | 0-1400   | 0.0-78.8           | 0.0-97.0   | 0.0-81.3 |
| Median | 3.4                               | 4.7     | 4.7     | 0.0                          | 1.0     | 2.0     | 463  | 550      | 425      | 18.0               | 32.8       | 27.8     |
|        | ALBINO MICE - Females (N=20)      |         |         |                              |         |         |  |          |          |                    |            |          |
| Range  | 1.5-6.5                           | 3.9-7.3 | 3.7-7.4 | 0.0-4.0                      | 0.0-5.0 | 0.0-5.0 | 0-1075   | 0-1475   | 0-1500   | 0.0-53.1           | 0.0-65.6   | 0.0-71.9 |
| Median | 4.7                               | 5.7     | 5.7     | 0.0                          | 1.0     | 2.0     | 650  | 725      | 800      | 12.1               | 17.2       | 12.3     |
|        | WILD MICE - Males (N=6)           |         |         |                              |         |         |  |          |          |                    |            |          |
| Range  | 4.8-8.0                           | 4.6-6.1 | 4.0-6.3 | 0.0-5.0                      | 1.0-5.0 | 3.0-5.0 | 250-575  | 300-775  | 375-1300 | 3.0-100.0          | 0.0-34.4   | 3.0-60.6 |
| Median | 6.5                               | 5.8     | 4.8     | 1.0                          | 2.5     | 4.0     | 450  | 600      | 613      | 37.9               | 18.2       | 9.3      |
|        | WILD MICE - Females (N=6)         |         |         |                              |         |         |  |          |          |                    |            |          |
| Range  | 4.3-6.6                           | 4.6-7.0 | 4.0-5.4 | 0.0-5.0                      | 2.0-5.0 | 0.0-5.0 | 350-1400   | 100-1200 | 100-1625 | 0.0-69.7           | 12.1-100.0 | 6.1-63.6 |
| Median | 6.0                               | 5.9     | 5.3     | 2.5                          | 3.5     | 5.0     | 675  | 650      | 875      | 48.5               | 31.8       | 18.2     |

In spite of the stability of individual performance in each behavior, there were no significant concordances among the individual performances in the four tests of each series (values of  $W$  in test series I to III were .249, .304, and .394 for albino males; .195, .341, and .348 for albino females; .140, .058, and .360 for wild males; .080, .197 and .167 for wild females). No animals presented a systematically high or low performance in all tests and several mice presented even quite contrasting performances in two or more behavioral activities. For example albino male AM09 had average performances in EX (3.0, 4.9 and 2.9 min of active exploration) and HD (9.4, 18.8 and 56.3% of pellets in the den), but very high PD (all cockroaches killed in the three tests) and nil BW performances. In contrast, albino female AF22 had very high EX scores (6.5, 7.3 and 7.4 min as active exploration), low PD (only one cockroach killed in test III) average BW (675, 1100 and 700 cm<sup>2</sup> of underground burrowing), and low HD (3.1, 3.1 and 9.4% of pellets in the den).

Sex differences occurred mainly in the albino strain. In the three series of EX tests females as a separate group showed a higher performance than males (values of Mann-Whitney  $U$  were respectively, 107.5, 99.0 and 108.0, all with  $p < .05$ ). Albino females also presented significantly higher BW performances in the second and third test series ( $U = 130.5$  and  $131.0$ , respectively). On the other hand, albino males presented significantly higher values in the second HD test ( $U = 124.5$ ). Among wild mice sex differences occurred only in the first BW test ( $U = 7.0$ ) in which females excavated larger burrows.

Significant differences between both stocks occurred mainly in the first test series with the wild animals scoring higher than albinos. In males significant differences occurred only in the first two EX tests ( $U = 12.0$  and  $25.0$ ). In females there were differences in EX, PD and HD with wild females having significantly higher performances in the first test series ( $U = 25.0$ ,  $23.0$  and  $28.0$ ). Wild females had also higher performances in the second PD and HD tests ( $U = 24.0$  and  $27.0$ ). In contrast albino females had higher EX scores in the third test ( $U = 30.0$ ). It was also observed that, in the burrowing activity, no differences between stocks occurred and that, even in the first series of tests most animals (14 out of 20 albino males, 17 out of 20 albino females and all the wild mice) dug underground burrows, some rather extensive (e.g., AM43, AF16, AF18, AF34, WF02, WF03 had burrows with areas = 1000 cm<sup>2</sup>). An evident difference between both stocks is the broader range of individual performance in albino mice, especially the occurrence of low performance. Among albinos, 21 out

of 40 animals had nil performances in two or all of the three tests of any behavior; among wild mice this occurred only with one out of 12 animals. This difference was statistically significant ( $\chi^2 = 5.68$ ;  $p < .02$ ).

Besides these quantitative evaluations our behavioral tests showed some characteristics that deserve description. In opposition to the high inter-individual variability of behavioral performance, there was greater homogeneity in the motor patterns observed in the diverse animals. In the exploration tests the mice typically showed hesitant locomotion during the first exits from the den compartment. The animals remained for longer periods of time in the tunnel with repeated to-and-fro movements. During the first exploration of the environment the animal almost crouched on the floor. In the predation test there were characteristics common to all animals that showed marked predatory motivation, namely, the insects were pursued, grabbed with the forepaws and bitten, usually in the head. In the burrowing test a rather fixed sequence of motor patterns occurred involving excavating with the forepaws, pulling the loosened soil beneath the body, followed by kicking the soil with the hindpaws. These patterns were observed even in albino mice in the first test series (i.e. during the first contact of the animal with the soil). The excavated burrow contained one or two large chambers which communicated with the environment through one long and narrow tunnel. The burrowing pattern in both stocks was either all (if a burrow was started a rather large excavation resulted) or none (the animal did not make an underground excavation). Animals removed volumes of soil which corresponded to many times their body volume. In wild animals, due to their smaller size and higher performance, this proportion was even more marked. For example, female WF02 weighing 14 grams removed about 4500 cc of soil, corresponding roughly to 300 times its body volume. In the hoarding tests there were also some typical motor patterns, namely, exploratory activity in the environment associated with hoarding of materials occurred in bouts. Often only one kind of material (food or nesting material) was transported during a bout. Individual animals differed not only in the total amount of transported material but also in their preference for food or non-food items. Hoarded materials were taken in the mouth and transported one-by-one to the den where they were left without any distinct arrangement.

## DISCUSSION

Our present results show that behavioral performance in mice is distributed over a wide range. Individual differences do not seem to be a consequence of casual variability since individual performance in a given behavioral activity tended to remain stable along the experimental period (60 days), as shown by the significant concordance coefficient among the three test series. Nor are these differences a consequence of some general factor such as the animal's level of activity since individual performance in different behaviors (measured in the same test series), showed no significant concordance.

There are references to IDs in mice and other rodents in the literature. In *exploratory behavior* marked differences among individuals have been described, both in mice (Wolfe, 1969) and rats (Tryon, 1934; Negrão & Schmidek, 1987; Hooks, et al., 1991). In *predatory behavior* of rodents marked individual variations are also described. Butler (1973) found a great variability in the cricket-killing behavior of mice. Polsky (1975) described great inter-individual differences in the locust-killing activity of the golden hamster. IDs were also reported in mouse-killing (Paul, Miley & Baenninger, 1971), frog-killing (Johnson, De Sisto & Koenig, 1972) and cockroach-killing (Negrão & Schmidek, 1987) behavior of rats. In *burrowing behavior* of mice, Dudek et al. (1983) described high inter-individual variability, even in some inbred strains. Also in wild house mice high variability in this behavior has been demonstrated (Bouchard & Lynch, 1989). Finally there are clear descriptions of IDs in *hoarding behavior* in rats (Negrão & Schmidek, 1987), golden hamsters (Polsky, 1975) and mice (Smith & Ross, 1953a) in which subgroups of "hoarders" and "non-hoarders" were described. IDs in nest building activity of mice have also been described (Lisk, Pretlow & Friedman, 1969). Several types of nest ("half-nest", "bird-nest", "pocket-pallet nest", "semi-circle nest" and "maternal-nest") differing greatly in size and complexity, have also been catalogued by these authors. Contrasting with the great variability in performance we found a remarkable homogeneity in the motor patterns of each behavior as already documented for exploration (Birke, D'Udine & Albonetti, 1985; Connor, 1975; Oortmerssen, 1971), and burrowing (Adams & Boice, 1981).

Our identification of sex differences in exploratory behavior is in accordance with the results of D'Amato (1986) who described higher frequencies of exploration in females. The role of the estrous cycle in this phenomenon is still uncertain. An increase of locomotor activity

in the early morning of the cycle has been described but more complex behavioral patterns such as "peeking" have not revealed such a relation (Guttman, Lieblich & Gross, 1975).

We have also detected sex differences in burrowing behavior with females tending to burrow more than males. Similar differences have also been described in rats (Boice, 1977; Negrão & Schmidek, 1987). Notice, however, that in both species the range of IDs produces wide overlapping in male and female performance.

In spite of the reduced number of wild mice in our experiment, some interesting contrasts to albinos appeared. The first important difference is the broader range of individual performance in albino mice, especially with respect to low performance. As described, 21 albinos out of 40 had nil scores in two or all of the three tests of any behavior whereas among wild mice this occurred only with 1 out of 12 animals. This picture conflicts with the considerations of Wolfe (1969). According to this author, due to their greater heterozygous characteristics, wild mice should present a greater inter-individual variability. Our data suggest that selective pressures in the wild life may actually reduce variability in behavioral performance (especially in the range of low performance).

The wild environment may be responsible for the higher exploration of wild mice in the first test series. In fact Henderson (1970) showed that mice reared in an enriched environment (a breeding cage containing several objects) show more exploration than control-groups of animals raised in conventional cages ("restricted environment"). In our test it is possible that this influence determines the initial differences between both groups as well as accounting for the gradual attenuation of these differences once both groups are kept in conventional individual cages. The greater exploration of wild mice may also be influenced by them having pigmented eyes. Dixon and DeFries (1968) showed that albino mice on average are less active than pigmented animals in an open field test. Obviously this possibility cannot be the only determining factor in our experiment since wild animals had explored more only in the first exploration test.

Both stocks also differed in predation performance which was higher in wild mice. It is probable that the previous experience of this group in insect predation have determined the difference. Notice in this respect, that the average predation performance of albino animals increased from test to test. Training influences on insect predation performance have been described in mice (Thomas, 1969) and in other rodents such as rats (Negrão & Schmidek, 1987), golden

hamsters (Polsky, 1977) and grasshopper mice (Baxter, 1979).

Albino and wild animals also differed in hoarding behavior. Wild females hoarded more in the first two test series. Environmental factors acting upon a genetic predisposition may have caused this difference. According to Bindra (1948), the amount of material that is hoarded by a rodent reflects the animal's previous experience concerning availability of the material and safety of the den in relation to the environment. If the environment is safe and food is abundant the food is consumed directly in the environment. Conversely the more the environment is dangerous and/or the food is scarce the greater will be the hoarding performance. Both these factors may have been acting on wild animals determining their increased transport of food pellets to the den in the first test series. Also, the richness of the original environment of wild animals may have influenced their hoarding activity (Manosevitz, Campenot & Swencionis, 1968).

If we compare individual performance in several types of behavior some interesting facts appear. Our animals showed a significant tendency to keep a similar performance in each one of the four behaviors along the three test series (in a period of three months for each animal). Contrasting with this stability there was no evident correlation between the performances in different behaviors in the same test series. Actually, some animals showed quite contrasting performances in different behaviors (tested on successive days). Thus it seems that in a given animal the motivational processes regulating performance in different behavioral activities tend to be independent of each other. The degree of specificity and fine tuning of these regulating processes may be illustrated by hoarding activity. Motivation for hoarding different materials seems to be specific and rather independent. From our observations, individual performance in food and non-food material-hoarding sometimes indicated many discrepancies, and different non-food materials were hoarded with different intensities. A similar differentiation among materials has already been described in mice (Smith & Ross, 1953b) and rats (Herberg, Pye & Blundell, 1972) for hoarding of food versus cotton balls, and in rats (Wallace, 1978) for the hoarding of different non-food materials. The process of independent regulation of different motivational systems turning each animal into an individual with a conspicuous response pattern to the environment may be adaptive. As already discussed for other species (Barash, 1976; Hirsh & McGuire, 1982; MacDonald, 1983), IDs increase variability with obvious selective advantages and may lead, in a social species such as the mouse, to a spontaneous division of activities. As a hypothesis one

might speculate that, based on their specific motivations, individual animals tend to assume specific roles in the group.

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## INTRODUCTION

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**The Inevitable Bond: Examining Scientist-Animal Interactions**, edited by Hank Davis and Dianne Balfour. New York: Cambridge University Press, 1992, 399 pp.

As comparative psychologists we all have worked closely with animals. How do we feel about our "subjects?" How do they feel about us? Why does it matter? The issue of the interactions that inevitably occur in our science between researcher and subject is examined by many of today's leading comparative psychologists in this nicely-produced volume edited by Hank Davis and Dianne Balfour. The book has twenty-four chapters; three chapters present the issue - the concept of bonding, and the rest are reports from researchers concerning their experiences, methodologies and approaches to the study of animal behaviour and physiology. The diversity of species studied (ranging from octopuses to chimpanzees) and of methodologies utilized means that the book has something to say to nearly all CPists. We reprint three reviews of *The Inevitable Bond* and a response by the authors. The reviewers raise a number of controversial issues, including anthropomorphism and the use of anecdotal evidence. We invite your responses to the reviews presented here, and to Davis and Balfour's book.

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Earlier versions of the reviews and response were published in the electronic journal *PSYCOLOQUY*. *PSYCOLOQUY* files can be retrieved by anonymous ftp to "princeton.edu" where they are archived under the "/pub/harnad/" directory.

## SHOULD SCIENTISTS BOND WITH THE ANIMALS WHOM THEY USE? WHY NOT?

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*The Inevitable Bond* (Davis & Balfour, 1992; Davis, 1993) is a useful and well-edited collection of original essays. Davis and Balfour's introductory remarks and the brief summaries they provide before each chapter are helpful for keeping the central theme -- scientist-animal interactions -- in focus. They and their contributors have produced a volume that is long overdue, one that forces scientists to come to terms with how they interact with the nonhuman animals (hereafter animals) they study, and why they interact in the ways they do. For some scientists this is a topic about which they would rather think than talk, but the many issues that need to be considered in studies of scientist-animal bonds will not disappear if they are ignored. And now they can no longer be ignored; *The Inevitable Bond* brings the issues to the table for much needed open discourse. The opposing views concerning the nature of scientist-animal interactions provide needed balance; there are those who want to use standardized techniques to reduce the confounding behavioral and physiological effects of human contact with the animals whom they study and those who accept the inevitability of the bonds that are formed between scientists and animals. The latter view is consistent with an approach in which the relationships can be used to make for better research; "better" means different things to different people, but the notion seems to boil down to practices that include expediting data collection, coming to understand those experiments or manipulations that are likely to work without stressing the animals to the point that they are no longer the animals on which one thinks one is working, reducing the number of animals to be studied,

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and providing less contrived explanations of the behavior patterns being studied. There seems to be more consensus among those who study social behavior and animal cognition than, for example, among behavioral physiologists, that the view to be exploited for the benefit of both the scientists and the animals is the one that accepts the inevitability of scientist-animal bonds (see the chapters by Pepperberg, Boysen, Oden & Thompson, Schusterman, Gisiner, & Hanggi, and also Lorenz, 1991). I strongly favor this position for a number of reasons that will become clear.

I agree with so much of the material in *The Inevitable Bond* that my review is more of an appeal for an increased willingness on the part of those who use animals in research to recognize that bonding exists, and for an attempt to use this to the mutual advantage of the scientists and the animals they use. This is not what all scientists believe, however, nor has this view enjoyed a lot of popularity for a long time; many still operate on the basis of what Rollin (1989) calls the "common sense of science," according to which science is a value-free, objective enterprise. Thus, Davis and Balfour's recognition that neither their book nor the main topic of interest would have been viewed favorably as few as 20 years ago is realistic. As Deborah Gordon (1992, p. 23) has pointed out, "The way that scientists see animals' behavior occurs... [in] a system embedded in the social practices of a certain time and place." Why has there been a change in attitudes concerning the discussion and analysis of scientist-animal interactions? There seem to be at least three reasons: (i) Many researchers are no longer embarrassed by the fact that they and the animals they use in research form tight social relationships; (ii) many scientists realize that the bonds formed with their research animals are likely to influence how data are collected, interpreted, and explained, and they want to learn more about this phenomenon in order to help their research; and (iii) many researchers recognize that they are responsible for the animals' welfare, and that their decisions about how to treat animals are informed by how they feel about the animals themselves. Davis and Balfour's main agenda concerns points (i) and (ii), although they do pay some attention to (iii). I will mainly be concerned with (iii), namely, how the inevitability of bonding with many/most of the animals one interacts with closely in a research setting bears on many animal welfare issues.

Many different species of animals (rodents, canids, monkeys, apes, birds, octopi, reptiles, pigs, goats, horses, pinnipeds, bears) are considered in *The Inevitable Bond*, and this is a strong positive feature; laboratory rodents, domestic dogs, and nonhuman primates are not the only animals with whom we need to be concerned (for a consideration

of many of the same issues discussed in this volume, see Gordon 's [1992] discussion of ant-watching). However, by studying the ways humans interact closely with familiar animals, a lot can be learned that can be used in studies of human encounters with other animals. For example, Clinton Sanders's (1993; Sanders & Arluke, 1993) work with domestic dogs supports the idea that intense emotional bonds between humans and dogs are central to contemporary social life. (Sanders also notes, in response to a skeptical critic of his work, that the people he studied were not "wacky and lonely people who are over-involved with their pets.") Sanders's work supports the notion (see below) that not allowing bonds to be formed with at least some animals, especially those who might expect a bond to be formed such as members of domesticated species, could be stressful to the animals and could affect research findings. Along these lines, Lorenz (1991, p. xvi) recalls an incident in which a hand-reared gosling was merely deprived of being greeted by the person who reared her "during an apparently harmless experiment." The deprived gosling became so stressed that Lorenz and his coworkers never repeated the experiment. Lorenz also points out that bonds can be formed as a result of very subtle and seemingly unimportant exchanges between scientists and animals, interactions about which we know little.

Among the common themes covered in *The Inevitable Bond* are anthropomorphism and anecdotalism, and questions such as "should humans form bonds with the animals they use in research" and "does allowing bonds to form between scientist and animal result in 'good' or 'bad' science?" In my view and that of others, anthropomorphism is not only an unavoidable part of behavioral research, but necessary if humans are to come to a better understanding of the behavior and mental lives of the animals they study. Even those who take care not to use anthropomorphic explanations usually resort to them anyway because they make the behavior of diverse animals accessible to humans (for a useful discussion, see Myers, 1991, pp. 200ff). Or those who frown on anthropomorphic descriptions or explanations might place objectionable terms such as dominating, submitting, helping, or retreating in scare quotations, and then tell readers that the words should not be taken literally but metaphorically. Occasionally they simply declare "Oh, you know what I mean." The great challenge to those who believe we can dispense with anthropomorphism is to develop other ways to describe and explain animal behavior and the mental and affective states of animals in a way that is as accessible to humans as anthropomorphic descriptions and explanations; this is a very difficult task (Bekoff, 1995). With respect to the main topic of Davis and Balfour's book, Lehman (Chapter 24) points out that using cumbersome terminology to avoid

using the term "bond" really doesn't help matters. Furthermore, he notes that (i) those who criticize others for being anthropomorphic are not necessarily being good scientists, (ii) the desire to be rigorous may lead one to overlook the fact that animals have bonded to the researchers, and that this bonding influences results, and (iii) for many of the animals who are used in research, not allowing them to bond may be stressful and may likewise influence results. A plethora of data show clearly that we are part of the picture, like it or not, and attempts to detach ourselves totally from the animals are fruitless. What is called objective science is not necessarily better science than science that is done with animals to whom one feels close.

Anthropomorphic terms should certainly be used with care (see Burghardt's chapter in *The Inevitable Bond* and Burghardt, 1991; Lorenz, 1991; and Bekoff & Allen, 1995), but I do not think that (i) anthropomorphic explanations are dangerous (Estep & Hetts, Chapter 2, p. 23), that (ii) the "dangers of anthropomorphism in science are widely known" (Estep & Hetts, p. 23) or broadly feared, or that (iii) anthropomorphism should be avoided at all costs. Thus, I do not agree with Estep and Hett's admonition that "Scientists must keep a constant vigil against anthropomorphic thinking and interpretation when performing animal research" (p. 23), if this vigil is motivated by a fear of the supposed dangers of anthropomorphism. (Kennedy [1992, p. 167] also fears anthropomorphic tendencies because they fly in the face of objective, hard science, but he then goes on to claim, in the absence of any hard data, that anthropomorphism is genetically programmed into humans, [but] "that does not mean that the disease is untreatable".) Perhaps it is Estep and Hett's confused conception of anthropomorphism that is at the root of their worry. For example, they write (i) about "anthropomorphic or zoopomorphic behavior" (p. 15), (ii) about "an organism [is] behaving anthropomorphically," and (iii) that "anthropomorphism and zoopomorphism are little more than descriptions of the behavior of organisms toward one another" (p. 15). I suggest that interested readers see Hugh Lehman's chapter in *The Inevitable Bond* and John Andrew Fisher's papers (1990, 1991) for more informed discussions of anthropomorphism. Lehman correctly notes that anthropomorphism entails attributing "a human affective, emotional, or cognitive capacity to animals" (p. 388); animals do not behave anthropomorphically, nor are there anthropomorphic behaviors.

Anecdotalism also should not receive the bad press it frequently incurs. My colleague Dale Jamieson quips that "the plural of anecdote is data," but one can also correctly claim that anecdotes are, in and of themselves, data; they just may not be the sorts of data with which some

feel comfortable. But few are happy to stop with the collection of anecdotes; most people realize that anecdotes provide the material out of which more rigorous research grows, in all fields, including both the behavioral and the physical sciences (see Fentress's discussion, p. 44). The claim that anecdotes are useless is unjustified. The species of animals with which people form close bonds are diverse, as noted in many chapters, but do humans form bonds with some species more readily than with others? And, if so, why? Scott (p. 89) writes: "It should be easier for a human to become attached to another mammal than to an insect. Although I have no proof that this is true, it should be easier to extend human-human relationships to similar species than to distantly related ones." No one seems to have any hard data concerning this thought. I have often been asked if I felt closer to the domestic dogs, coyotes, or wolves I studied than to the birds (Adelie penguins, western evening grosbeaks, house finches, juncos, cowbirds) I observed, or whether I identified more with the former than the latter. At first these questions made no sense to me, but I came to learn that most people who asked them had already decided that I felt closer to the coyotes and wolves than to the birds. They were wrong. I did identify differently with the canids than I did with the birds, but I did not feel less close to the birds than the canids.

Another issue that receives some attention in *The Inevitable Bond* is the naming of research animals. Although some believe that naming animals is a bad idea (because named animals will be treated differently, less objectively, than numbered animals), others believe just the opposite, that naming animals is permissible and even expected when working closely with at least certain species, especially with the same individuals over long periods of time. Early in her career, Jane Goodall had trouble convincing reviewers of one of her papers that naming the chimpanzees she studied should be allowed. Goodall refused to make the changes they suggested, including dropping names and referring to the animals as "it" rather than "he" or "she," or "which" rather than "who;" her paper was published (Montgomery, 1991, pp. 104ff). It seems noteworthy that researchers working with nonhuman primates and some cetaceans usually name the animals they study; we read about Kanzi, Austin, Sherman, Koko, Phoenix, and Akeakamai (and see pictures of them with their proud human companions; Linden, 1993). We also read about Alex, an African gray parrot (Pepperberg, Chapter 11). Yet most people do not seem to find naming these individuals to be objectionable. Is it because the animals who are named have been shown to have highly developed cognitive skills? Not necessarily, for these and other animals are often named before they are studied intensively. Or, in the case of most

nonhuman primates, is naming permissible because these individuals are more similar to humans than are members of other species? Why is naming a rat or a lizard or a spider more off-putting than naming a primate or a dolphin or a parrot? We need to know more about why this is so.

It is also worth asking whether there is any relation between the resistance to forming bonds with and to naming animals, on the one hand, and the type of research in which one is engaging, on the other. Do field workers differ from laboratory workers? Do those who restrain, isolate, or shock animals differ from those who merely observe them? Do those who have to kill animals differ from those who do not? Does one's familiarity with a given species influence one's resistance or desire to form bonds and to name the animals with whom one works? These are, of course, empirical questions that need further study. However, as Serpell (1986) has noted, increasing the distance between themselves and nonhuman animals is a common practice among scientists and nonscientists. Among the devices used are objectifying animals by referring to them with "it" and "which," and using terms such as collecting, euthanizing, sacrificing, and culling to refer to killing (see Lynch, 1988; Verhoog, 1991; Bekoff, 1993). Likewise, Davis and Balfour point out, bonding with an animal may make it impossible to subject them to different forms of treatment (pp. 1-2). Dewsbury also mentions that an interest in scientist-animal interactions may lead to less exploitative studies (p. 27). Furthermore, for those who want to learn more about animals' mental states, it seems reasonable to treat animals as subjects rather than objects (Bekoff & Jamieson, 1991; Bekoff, 1995; Jamieson & Bekoff, 1993).

Scientists also show different attitudes toward animals of the same species depending on whether they are encountered in the laboratory or at home. Rollin (1989) and others have noted that many scientists who name and praise the cognitive abilities of the companion animals with whom they share their home are likely to leave this sort of baggage at home when they enter their laboratories to do research with members of the same species. In addition, there are those who inform their laboratory research using anecdotes that stem from observations of their companion animals, with whom they are freely anthropomorphic (Rollin 1989). Based on a series of interviews, Phillips (1993) reported that many scientists socially construct a "distinct category of animal, the 'laboratory animal,' that contrasts with nameable animals (e.g., pets) across every salient dimension... the cat or dog in the laboratory is perceived by researchers as ontologically different from the pet dog or cat at home." Rajecki, Rasmussen, & Craft (1993) found that the ways

in which animals are labelled and categorized influences the level of tolerance with which people view different forms of mistreatment. All in all, bonding with animals and naming them seems to influence how the animals are viewed and treated, and I agree with those who believe that these practices should be exploited for the benefit of the scientists and the animals. It seems unnatural not to do so.

Calling animals by name and bonding with them are steps in the right direction for both the scientists and the animals; both sides will benefit greatly from a deeper examination and understanding of the nature of scientist-animal interactions. The knowledge gained by viewing animals as individuals and learning more about each individual's characteristics should work against wholesale species-centered decisions concerning animal welfare (Bekoff & Gruen, 1993). "Speciesists" make decisions about how humans (moral agents) are permitted to treat nonhuman animals (moral patients) based on species membership and not on individual characteristics (Ryder, 1975/1983, 1989). For example, James D. Watson, is quoted in 1993 for his sweeping dismissal of the animal rights movement, claiming that all those who are interested in how animals are used by humans want to "spend all our resources making monkeys happy," and further, that "I don't like monkeys." Does Watson mean all monkeys? I am sure a lot of other people also wouldn't like monkeys if the only monkeys with whom they had had contact were caged ones. Perhaps there are some individual monkeys Watson would come to like? Some scientists may not like (or want to bond with or name) the individual animals on whom they work, and they may extend their dislike to all members of the same species, but their personal views cannot settle questions about the morality of animal use.

*The Inevitable Bond* is one of those rare finds, an edited volume that is worth reading in its entirety. One area that needs much more detailed study concerns the bonds, and the effects of the bonds, that develop between field researchers and the animals they study. Nancy Caine's informative chapter is a good place to begin for those interested in scientist-animal interactions in the wild (other references can be found in Bekoff & Jamieson [1991, 1995]). A combination of information from studies on captive and wild animals is needed to come to a fuller understanding of the many different aspects of scientist-animal interactions.

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## EFFECTS OF EXPERIMENTERS ON THEIR ANIMAL SUBJECTS CAN BE THE SOURCE OF VALUABLE KNOWLEDGE

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The focus of this volume (Davis & Balfour, 1992) is on the bond that forms between experimental subject and experimenter. The term bond is used quite loosely to mean (a) the reaction of experimenter to subject, (b) the reaction of subject to experimenter, and (c), more specifically, an emotional attachment either way. The editors make it clear at the outset that they are not interested in identifying experimenter contamination effects. For example, cases in which the behavior of the experimenter provides an inadvertent cue to the animal (i.e., the so called Clever Hans effect) are prominently included in experimental psychology textbooks. Similarly, the dangers to the objective collection of data that can result from experimenter expectancies are well known and students of experimental psychology are told to avoid them by making themselves (as experimenters) blind to the experimental treatment. The more interesting facet of experimenter expectancy, according to Davis and Balfour, is the direct effect that such expectancies have on the animal's behavior. In other words, experimenter expectancies not only can affect the observation of behavior but they can affect, as well, the behavior itself.

Identification of the bond as the focus of research interest provides a fresh approach to experimenter-subject interaction. Because traditional approaches stress the contamination of findings that can result when data collection affects the animal's behavior, experimenters have been admonished to avoid, or at least reduce, such interactions. Hence the tradition in experimental psychology has been to objectify the collection

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of data by automation (e.g., by using operant chambers in which responses are recorded by microswitch and reinforcers are provided automatically). The authors of a small number of the chapters in this volume (e.g., Chapters 8, 9, and 10) seem to present the traditional argument for isolation of experimental subject. But authors of the majority of chapters view isolation of the subject from the experimenter as a practice that obscures the observation of valuable behavior. For them the subject-experimenter bond is worthy of study in its own right.

Estep and Hetts (Chapter 2; see also Chapter 3 by Dewsbury) provide an excellent analysis of the various roles that an experimenter can be perceived by an animal to play: predator, prey, symbiont, or conspecific. The authors go on to identify specific emotional mechanisms that can enter into the bond and they suggest how analysis of the particular bond can lead to a better understanding of the animal. Paradoxically, they note that the better the behavior of the animal is understood, the less likely one is to anthropomorphize.

The most adventuresome perspective on the experimenter as coactor is taken by Fentress (Chapter 4) and by Burghardt (Chapter 23) who suggest that the only way to hope to understand the complexities of human-animal interaction is for the experimenter to interact with the animal in a variety of contexts and to observe the behaviors that are evoked. These authors admit to the possible subjectivity of their observations, but justifiably note that one must first observe a behavior before it can be objectively studied. Their approach exemplifies one that is new to the study of animal behavior, in which speculation based on careful observation is used to generate testable hypotheses about underlying mechanisms. There are many (well known) pitfalls to such an approach but the potential rewards make it well worthwhile.

Somewhat closer to the laboratory techniques typical of traditional experimental psychology are procedures described by Pepperberg (Chapter 11) and Boysen (Chapter 12). These chapters capitalize on what the authors have learned from their interactions with animals to facilitate cognitive assessment. Pepperberg's use of intrinsic rewards (the parrot is given the object named) apparently helps the animal to differentiate among responses. The model/rival technique, also used by Pepperberg, takes advantage of the parrot's predisposition to learn from social example. Similarly, Boysen's sensitivity to the social and attentional behaviors of her chimpanzees allows her to demonstrate remarkable learning in these animals.

In edited volumes, an attempt is made to provide the reader with an overview of research in a field. Such a format often has the advantage of providing broad coverage of research presented by researchers having

different theoretical perspectives and in which different methodologies are explored. A drawback often present in edited volumes, however, is an unevenness of writing, as well as a lack of adherence to the central theme of the volume by some of the contributors. It is a challenge to the editors to organize the volume into a coherent whole. As is the case here, this is especially difficult when the editors are attempting to define a new area of research by bringing together the writings of a group of researchers whose work comes from quite diverse disciplines. One needs to view such an edited volume as a means of communicating to researchers outside the area that such a field of study exists and to those engaged in this field of research, that there are others who share similar interests. This volume should serve the purpose of identifying a field of study and acting as a heuristic for further research.

The editors provide brief introductions to each chapter and these were useful in tying the chapters together. Along these lines, however, the editors could have provided additional structure by dividing the book into sections. Although such divisions are sometimes artificial, they often provide context to a set of related chapters. Another means of tying an edited volume together is through reference to authors' research cited in more than one chapter. Unfortunately, no author index was included in the volume.

The last chapter in the volume provides a useful epilogue that mirrors the editors' initial chapter. Written by Lehman, a philosopher, it expresses a sentiment, becoming more popular in the area of experimental research, that a reliance on the objective description of behavior and avoidance of conjecture about underlying mechanisms of learning, cognition or emotion may have as many drawbacks as the anthropomorphism so often criticized by traditional animal researchers.

## ACKNOWLEDGEMENTS

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## WHO WATCHES THE WATCHMEN?

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*The Inevitable Bond* is a thoughtful look at how students of animal behaviour relate to their subjects. Cohesive and rich, it is a fine example of what a multi-authored book should be. Davis and Balfour provide introductory notes to each chapter, and have obviously taken great care in placing similarly themed chapters together. One paper's ideas are picked up and reiterated in others, becoming powerful motifs.

One of *The Inevitable Bond's* strongest themes is the use of anecdotes as a source of scientific information (see also Bekoff, 1993). Fentress (Chapter 4) puts the case for anecdotes forcefully and convincingly. He shows that deep insights into an animal's behaviour can be gained by watching "one-off" incidents, although he cautions that such observations must be supported by more controlled and rigorous testing (also Duncan, Chapter 18). Echoes of Fentress's argument are found throughout the book. Virtually every chapter contains vivid anecdotes; anyone teaching animal behaviour should find stories worth telling their students. Even Summerlee's chapter on the neurophysiology of arousal ends with an informal observation of how he could hear a rabbit's physiological response (by hooking a speaker into the equipment) and tell if it had been disturbed.

Even though casual observations are often striking and can be a gold mine for new ideas, there is practically no place to publish anecdotal information. Natural history has seen better times, and the excitement of ethology often seems lost amid charts and tables (Kortlandt, 1990). By neglecting anecdotal information, we do ourselves a disservice in several ways. First, we can end up avoiding the things that first attracted us to animal behaviour (Crowell-Davis, Chapter 20). Secondly,

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we lose knowledge about behaviour that can serve as a platform for other types of research (Bullock, 1993). Worst of all, because so many of these informal observations inspire and guide researchers, ignoring anecdotes can fundamentally distort our understanding of the scientific process. For example, several authors say their data collection was *dependent* on their experience, or a particular working relationship with their animals. After working with snakes for years, Burghardt can elicit attack behaviours that other researchers can't (Bowers and Burghardt, Chapter 16). Boysen (Chapter 12) says that a stable social environment for her chimps is "crucial" for gathering data. Furthermore, chimps not only form relationships with their researchers, but may form particular kinds of relationships, differentiating between "playmates" and "mentors." Indeed, some people seem to be more popular with the chimps than others, but this was not investigated systematically (Oden and Thompson, Chapter 13). Finally, there is Burghardt's moving account of how only one (out of four) of his graduate students developed a relationship with two bear cubs that permitted him alone to run certain behavioural tests (Chapter 23).

Such stories clear the path to a fundamental epistemological problem: what constitutes good evidence? Good scientific evidence is supposedly repeatable by anyone. If, however, only a rare individual is able to gather certain sets of data, what should we make of the evidence then? Even worse, what if we aren't told the data were dependent on an unusual relationship between experimenter and animal, because the information was omitted as "too anecdotal"?

A second key theme in *The Inevitable Bond* is that animals can be incredibly sophisticated processors of information. As a simple example, many animals can recognize individual humans. This is amply documented in many chapters dealing with mammals (Dewsbury, Chapter 3; Fentress, Chapter 4; Boysen, Chapter 12; Crowell-Davis, Chapter 20; Schusterman et al., Chapter 21; Burghardt, Chapter 23). More unexpectedly, reptiles (Bowers and Burghardt, Chapter 16) and octopuses (Mather, Chapter 15) can also recognize people and remember them for weeks or more.

Besides recognizing us, animals can recognize what we are doing. Consider the well known story of Clever Hans. The lesson normally taken is that experimenters should beware inadvertently cueing their subjects. This seems to be why Davis and Balfour (Chapter 1) declare the Clever Hans effect outside the book's scope, but Crowell-Davis (Chapter 20) argues that a subtler point is often missed: animals can be very good at figuring out another species' behaviour. Clever Hans attended and reacted to human gestures that other people weren't even

aware of. Even when people realized they were cueing Clever Hans, they could not control those signals. The systematic study of interspecific signalling has been neglected, despite its impact on research. For example, when Pepperberg is working with her parrot, Alex, she and her colleagues must act like the task is interesting to them, or Alex is likely not to "pay attention" and start preening (Chapter 11). Dogs make excellent companions because humans unknowingly act like the dominant member of a pack (Ginsburg and Hiestand, Chapter 6). Worse, we often have little idea *what* animals might attend to. A standing human wearing gloves induces more fearful responses in pigs than a crouching, bare-handed one, for instance (Hemsworth et al., Chapter 17). Chickens are less stressed being caught by a machine than by a person (Duncan, Chapter 18).

Conversely, we seem to decipher animal signals erratically. There are strong examples of misinterpretation. Dolphins' gaping ("smiling") is actually a dominance challenge (Estep and Hetts, Chapter 2). Head nodding by some seals is an aggressive signal that some trainers unwittingly rewarded, hoping to use it in performance (Schusterman et al., Chapter 21). Nevertheless, Schusterman et al. make the counterpoint that sometimes the only way one can work with a species is to understand its social cues. The taller of two rearing male elephant seals becomes dominant; consequently, scientists can work fairly safely among them — even though the animals weigh over 20 times what a human does — because we stand higher than seals can rear (Chapter 21). Similarly, Burghardt's students had to fight off dominance challenges by their bears (Chapter 23).

Such interspecific signalling between scientist and animal is a prime example of how experimenter effects can form a continuum (Davis, 1993). As our closest phylogenetic relatives, chimps share much of our behavioural repertoire and so recognize many of our cues (Boysen, Chapter 12). On the other hand, although octopuses can recognize individual humans (Mather, Chapter 15), their ability to decipher human signals is probably limited. Likewise, we have an impoverished ability to appreciate the "mindset" of octopuses, which seem like something from science fiction. This may be why octopuses' behaviour frequently seems directly aimed at frustrating their keeper (personal observations).

Given the subtlety of experimenter effects, one may think that the most rational plan is to not interact with animal subjects. A few chapters expound this conventional wisdom and detail how bonds can be minimized. Baum and Hiestand (Chapter 14) talk about the sort of automated testing boxes championed by B. F. Skinner. Boccia et al. (Chapter 9), however, note that handling effects are hard to eliminate

when animals are kept for years and used in many experiments. Obviously, in many cases, reducing the number and intensity of interactions with animal subjects is totally appropriate, as it will alleviate both handling effects and stress on both the subjects and scientist (Duncan, Chapter 18). Paradoxically, one way to do this is by cultivating a relationship with the animals. Reinhardt details how, by developing a bond with their monkeys, they no longer had to fight to take blood samples (Chapter 10). Such an approach — working closely with animals with the express idea of forming relationships with them — is far better represented in *The Inevitable Bond* than the usual practice of shirking contact. Although the book doesn't convince me that it is impossible to avoid bonding, it does show how much research is *dependent* on animal-human bonds, as I noted above. Some scientists intentionally decide to work closely — almost intimately — with their animals, thereby exploiting a bond. Pepperberg (Chapter 11) has based her research program (effectively a decade-long and ongoing case study) on her interactions with one subject, her parrot Alex. This situation seems to be the norm in primate studies, represented here by Boysen (Chapter 12) and Oden and Thompson (Chapter 13). Not all experimenters can manage this amount of commitment to their animals (e.g., Thompson, 1976), but for those who can, there are substantial gains to be had.

If there are profits to be made by bonding with our animals, it's worth asking, "What *can* we bond with?" Obviously, sustained fear is not conducive to forming the productive relationships chronicled above, and Caine (Chapter 22) found that her monkeys simply did not habituate to the presence of humans, even after repeated exposures. Mather (Chapter 15) also implicitly raises the issue: If we can bond with octopuses, couldn't we conceivably bond with *Aplysia* or locusts or jellyfish? There are no real answers here, merely impressions. Personally, while I like the sand crabs and other assorted "crunchies" I study, it would stretch the word beyond its breaking point to say I "bond" with them. I doubt that my interactions with them change, in any significant or unforeseeable way, their locomotion which intrigues me so. But that's one of the beauties of a book like *The Inevitable Bond*: it makes you consider those sorts of questions.

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## A POSITIVE RESPONSE TO 'THE INEVITABLE BOND' WAS NOT INEVITABLE

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The consistently positive tone of the reviews of *The Inevitable Bond* suggests that the timing of our book could not have been better. Obviously, *The Inevitable Bond* addresses issues that many of our reviewers feel were hitherto neglected. However, the fact that there are relatively few substantive disagreements with the book's content could suggest that we are simply preaching to the converted. If this is indeed the case, it would seem that the conversion occurred at a rather well attended and secret ceremony. Given the controversial nature of some of the chapters, we expected to see some significant rebuttals. But the lack of conceptual or logical opposition has occurred not only in the reviews published to date here, in *PSYCOLOQUY*, and elsewhere, but also in the response to colloquia presented by one of the authors (Davis). Prior to the publication of *The Inevitable Bond*, there was a palpable resistance to a number of the book's fundamental premises. What has happened to this opposition? Among the kinder adjectives previously heard were "heretical" and "trouble-making". No one to date has carried such opposition into print. This validation has taken us by surprise. In essence, the strongest criticism in these reviews has been technical in nature; e.g. our lack of an author index (guilty as charged). No one has come forward to say things like "Scientists do NOT - by virtue of their repeated interactions with animals - affect the nature of the data they collect." Moreover, at no point in any review published to date, has there been a cry of "Much ado about nothing." Rather, the message

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seems to be "Yes, these are legitimate concerns and it's about time we formalized the treatment of this issue." This is what we hoped *The Inevitable Bond* would accomplish. If critical reaction to date is typical, we will have more allies than we expected in the battle.

The structure of the book has been both praised and criticized. Faulkes compliments us on the coherence we provide to the book's 24 chapters, organizing them into "powerful motifs." Zentall, on the other hand, suggests that we might have grouped the chapters along more explicitly defined themes.

We hope Bekoff is correct in his belief that *The Inevitable Bond* will "force scientists to come to terms with how they interact with the nonhuman animals they study." Bekoff himself reiterates many of the points addressed in *The Inevitable Bond*. For example, the idea that not allowing bonds to form with certain animals may in and of itself represent a significant stress which may in turn influence research is a theme which several authors addressed (e.g. Kostarczyk, Lehman). Such potential stress should be a source of concern for any scientist dealing with domestic species or primates.

Bekoff discusses anthropomorphism at length and emerges as a clear advocate for the use of anthropomorphic terms. As such, he takes issue with Estep and Hetts' admonition that "Scientists must keep a constant vigil against anthropomorphic thinking and interpretation when performing animal research." Bekoff also criticizes Estep and Hett's "confused conception of anthropomorphism." In fact, their discussion of anthropomorphism occurs as part of a review of Hediger's concept of assimilation tendency and his use of the terms anthropomorphism and zoomorphism. The topic of anthropomorphism is clearly a difficult one. Most of us learned early in our training to regard "anthropomorphism" as an obvious taboo for the rational, objective scientist. Any hint of anthropomorphism was tantamount to identifying oneself as uneducated or unsophisticated. Shifting from this traditional perspective to the "critical anthropomorphism" advocated by Burghardt will not be easy. How do we train young scientists to avoid the sort of uncritical anthropomorphism which leads to interpreting the dolphin's gape as a smile, yet encourage them to form bonds with their research animals and recognize when anthropomorphic explanations are appropriate? At the very least, we must recognize that "critical anthropomorphism" requires a sophisticated understanding of species-specific communication systems and how signals are used in intraspecific social relationships.

We hope that Bekoff's comments on anecdotes do not give readers the impression that *The Inevitable Bond* gives "bad press" to anecdotes. In fact, the viewpoint espoused by Bekoff is expressed throughout the

book, but most notably in the chapter by Fentress. Again, we have shunned the categorical rejection of anecdotes for a more open-minded, yet critical use of the strategy. It is virtually impossible to work closely with animal subjects and not come away with anecdotal data. However, these anecdotes are not an end in themselves. The use to which they are put within the scientific process is a major theme of our book.

Bekoff, along with a number of our contributors, wonders whether humans form bonds with some species more readily than others. Most researchers assume that humans are more likely to bond to similar species in part because we are more likely to recognize similarities in signals used for intraspecific and interspecific communication. Bekoff's own research experience with both canids and birds suggests that this assumption may be overly simplistic. Clearly, the answers to such questions will remain speculative until they have been subjected to more rigorous scrutiny.

One intriguing topic which Bekoff discusses, but which our book dealt with only in passing, is the correlation between the type of research and attitudes towards animals. Many ethologists are quick to assure people that the research they do has little in common with that of scientists whose research requires restraint, surgical intervention, and "sacrificing." There is an implicit assumption that people who really like animals simply don't do certain types of research. Needless to say, such feelings are rarely expressed without eliciting emotional rebuttals. Clearly, this is an area worthy of investigation by those interested in the psychology/sociology of science.

We agree completely with Bekoff's call for more detailed study of the bonds (and effects thereof) that develop between field researchers and the animals they study. One of the more frustrating aspects of editing this book was a reluctance to contribute by a number of field researchers. Over and over, we talked to field researchers who insisted that they didn't have anything to contribute, but then regaled us with anecdotes on the subject. Caine's results will no doubt make many a field researcher reevaluate just how "habituated" their subjects are to the presence of a human observer. One of us (Balfour) remembers only too well her apprehension when some of her ground squirrel subjects (usually yearlings) appeared to be going out of their way to forage in her vicinity. She also remembers the reluctance of her colleagues to explore such a possibility.

In his praise of Estep and Hett's chapter, Zentall refers to their observation that the more knowledge scientists have about the natural behaviour and behavioral capacities of the animal, the less likely they will be to anthropomorphize. As with many of the observations based

primarily on anecdote in *The Inevitable Bond*, this hypothesis has yet to be rigorously tested. Zentall also emphasizes one of the more radical suggestions of our book - that it be a phyrrie victory to hide behind automation in order to avoid bonding effects. In some situations, it is plainly worth the risk to interact with the animals in order to expand our understanding of the animal as well as our subject matter. Zentall has provided an excellent summary of the risks and rewards of this approach, which is typified in the chapters by Burghardt and Fentress.

Faulkes raises an important issue which, in retrospect, might have been addressed more directly in the book: the problem of what constitutes acceptable scientific evidence when working with animals. The complexities of relationships between scientists and animals may mean that certain results are unlikely to be repeatable without a specific relationship between scientist and animal. There are numerous examples of this possibility in our book (e.g. research by Burghardt, Pepperberg, Boysen). The fact that details about a relationship between scientist and animal are rarely included in published papers makes repeatability that much more unlikely. If nothing else, we would hope that our book encourages scientists to describe their interactions and relationships with their subjects as honestly as possible. Such descriptions should be considered an important component of any paper's methods section.

Faulkes also emphasizes the importance of understanding what our animals are attending to. As several of the examples in the book illustrate, human assumptions about animal perceptions are often misguided. Here is a prime example of the fine line between critical and naive anthropomorphism: most humans would undoubtedly assume that being caught by a machine would be more stressful to a chicken than being caught by human hands, but, as Duncan points out, they would be wrong. We hope the ideas presented in *The Inevitable Bond* will provide a stimulus for future work and provide a counterbalance to the prevailing reluctance to acknowledge the importance of scientist-animal interactions.

In sum, we are pleased, if a bit surprised, at the consistently positive peer review our book has evoked. Many of our contributors took professional risks in providing the information that appears in *The Inevitable Bond*. As Bekoff notes in his review, the message of our book is a topic about which many scientists would rather think than talk. In truth, our experience was even more conservative. It appeared that thinking about these issues was itself not a comfortable mode for some colleagues. We believed the time for reenacting the emperor's new clothes was past. It was for this reason that we wrote *The Inevitable Bond*: to stimulate discussion and promote further research. The

responses of our colleagues suggest that our perception was not unique. Indeed, it appears that a number of commentators have picked up the torch and run with it even further in their reviews that we did in our book.

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